

Armed conflict and schooling in Rwanda: Digging deeper

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Abstract: Investigating the impact of armed conflict on schooling in Rwanda, we present four key findings. First, we find a strong drop in schooling, both when using DHS data and when relying on two waves of population census data bracketing the violence. Second, in contrast to previous findings, we show that there is no leveling off, i.e. the drop is not stronger for non-poor and boys. Third, we demonstrate that the armed conflict caused a drop both in primary and secondary schooling attainment, be it through different channels; the drop in primary schooling driven by slower grade progression and increased drop-outs, while the drop in secondary schooling mostly due to a decline in school initiation. Finally, our results reveal a spatial mismatch between commune-level genocide intensity and the drop in schooling. We test for several potentially confounding factors, but find that none of these factors can fully account for the mismatch. We conjecture that the impact of armed conflict on schooling in Rwanda was nationwide, both because the disruption caused by the genocide affected every corner of the country and because - besides the genocide - other forms of violence took place in Rwanda in the nineties.

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1. Introduction

Micro-empirical work on the impact of conflict on schooling mostly rotates around two main questions: what is the overall impact of armed conflict on schooling? Does the impact vary across different strata of the population (e.g. boys vs girls)? The answers provided so far have been mixed. As expected, most studies found a negative impact on schooling, e.g. Swee (2009), Rodriguez and Sanchez (2012), Akresh and de Walque (2011), Shemyakina (2011), Chamarbagwala and Morán (2011). Other studies, such as Arcand and Wouabe (2009) and Valente (2011), estimated an overall positive effect. The evidence is even more mixed when it comes to the heterogeneity of these effects, for instance across gender: in some cases researchers estimated the impact to be stronger for boys (Akresh and de Walque, 2011; Justino et al., 2013), while in other cases they found it to be stronger for girls (Shemyakina, 2011; Chamarbagwala and Morán, 2011).

There are two main reasons behind these substantial differences. The first relates to the specific setting of the case studied. The actual impact of conflict on education depends on the relative importance of the specific supply and demand factors for education at work.¹ These factors, in turn, are determined by the context studied, which varies from case to case. First, armed conflicts differ in intensity, duration, geographic spread, and in the nature of the destruction - taking a large human toll or mostly destroying infrastructures. Second, the socioeconomic context plays an important role, for instance in terms of coping mechanisms that the households have at their disposal, or the prevailing social norms concerning child labor and

¹ Supply factors include the destruction of schools, the targeting of teachers and a decrease in the government budget for education, while demand factors include population displacement, youth enrollment in the military or rebel groups, limited mobility, changes in family structures (e.g. orphanhood), reduced life expectancy (affecting expected returns on human capital investment), increased poverty, and health and nutritional factors (affecting school performance and attendance). Justino (2010) and Buvinic et al. (2013) provide an overview.

gender-discrimination. Third, post-war policies may vary, depending on type and amount of aid flows and on the capacity and nature of the post-war government in place.²

The second set of reasons underlying the mixed evidence relates to the nature of the empirical analysis. Results may depend on the specific empirical model adopted as well as on the type and quality of the conflict intensity measures used, which can range from proxies such as dwelling destroyed to more detailed data on the number of battle events and human rights violations. Furthermore, the household survey data used in the analysis may or may not be representative at the national level or – more commonly – may not be representative across a number of characteristics, such as gender and separate income strata, or at the level of small administrative units.

This paper provides a contribution to the existing literature on conflict and education along both these dimensions. First, it provides novel evidence concerning the effects of armed conflicts and its underlying channels in the specific context of Rwanda, a country located in Africa's turbulent Great Lakes Region and characterized by a long and complex conflict cycle in the nineties, which included the 1994 genocide, civil war in 1990-92 and 1994-98, revenge killings, (counter-)insurgency operations in 1994-98 and a massive refugee crisis, and in which an estimated 1,000,000 lives were lost.³ Second, this paper provides insights into the role of the

² The relevance of the specific setting at hand becomes apparent when considering for instance the two studies mentioned above that found an overall positive effect of armed conflict on education. Valente (2011), focusing on the Nepalese civil conflict, explains her finding of a (small) positive effect in terms of the relative low intensity of the conflict and of the strong position of the Maoist insurgency against gender-discriminating traditions, which brought an increase in women's education. Arcand and Wouabe (2009) refer instead to the characteristics of the post-war labor market in Angola, with low wage for non-skilled labor and high expected returns to investment in education, to explain prolonged schooling of cohorts affected by the civil war.

³ It is estimated that the number of Tutsi killed during the genocide lies between 600,000 and 800,000, and that only 25% to 30% of the Tutsi population survived the 1994 genocide (Prunier, 1998; Verpoorten, 2005). The genocide targeted also moderate Hutu and an unknown number of Hutu became victims of revenge killings, other forms of violence, or fell victim to diseases in refugee camps (Des Forges, 1999). It is likely that the total death toll of all events related to the conflict cycle in the nineties amounts to 1,000,000 (Verpoorten, 2012). Moreover, it is

data used and the applied empirical strategy, by performing a broad replication of another study that addressed the same research questions in the same setting, but using different data and empirical models.

In particular, the paper starts with a replication of the widely cited study by Akresh and de Walque (2011)⁴ - from now referred to as AdW. AdW find that the Rwandan conflict caused an 18.3% drop in primary schooling, with the drop being stronger for boys and non-poor than for girls and poor. They attribute the drop in schooling to the genocide, by showing that the drop was more severe in regions where the genocide was more intense. In our replication of AdW, we introduce three main changes to the analysis. First, we make use of a larger dataset. Whereas AdW rely on two waves of Demographic and Health household Surveys (DHS, 1992 and 2000), we use two waves of population census data (1991 and 2002). Both data sources bracket the main conflict events in Rwanda, but whereas the DHS is representative at the province level, the census data is representative for much smaller administrative units, for different income strata, age groups, and across gender. Secondly, in contrast to AdW, we include all constitutive components of the interaction terms when studying the heterogeneous impact across gender and income. Finally, we use finer conflict measures, at the level of 145 administrative communes instead of the 11 provinces. Our results confirm a large overall negative impact on schooling, but not its variation across gender and income strata. Also contrary to AdW, our results indicate that the drop in schooling cannot be attributed to the localized impact of the genocide.

estimated that approximately 2 million people were displaced, and the transitional justice system for genocide suspects led to the imprisonment of more than 100,000 civilians.

⁴ According to Google Scholar, as of January 2014, the 2008 version of Akresh and de Walque's working paper was cited 118 times. The paper exists as an IZA discussion paper (IZA DP No. 3516, 2008), a World Bank Policy Research Paper (WPS4606, 2008), and a Households in Conflict Network Paper (HiCN Working Paper 47, 2008). The version we refer to is the latest update that is available on the personal website of Richard Akresh, dated February 2011.

Besides the broad replication of AdW, we provide two novel pieces of evidence. First, thanks to the rich data at our disposal, we are able to study for the first time the different impact of conflict on primary and secondary schooling, and the relative importance of three channels underlying this impact, i.e. school initiation, grade progression and drop-outs. Second, carefully investigating the reasons underlying the spatial mismatch between commune-level genocide intensity and the drop in schooling, we argue that the impact of armed conflict on schooling in Rwanda was nationwide, both because the disruption caused by the genocide was felt in every corner of the country and because - besides the genocide - other forms of violence took place in Rwanda in the nineties.

In the next section, we demonstrate the reliability of the Rwandan population census data, by showing that the census provides an exact match with population data from another source, and that the basic result of AdW - an 18.3% drop in schooling - can be replicated almost perfectly. Section 3 looks at the variation of the estimated drop across income and gender, providing evidence. In Section 4 we study the sensitivity of our results across the choice of age groups and quantify the drop in primary versus secondary schooling. Section 5 examines the relative role of school initiation, slow grade progression and dropouts. In Section 6 we demonstrate that the spatial distribution of the drop in schooling cannot be attributed to the localized effects of the 1994 genocide. Sections 7 and 8 investigate why this is the case. Section 9 concludes.

2. The reliability of the Rwandan population census data and the replication of the basic result of AdW

The DHS is firmly established in empirical research as a reliable source of information, mainly because of its standard and transparent approaches for data collection, cleaning and coding. In contrast, population census data do not enjoy a good reputation because the collection method and practice vary across countries and census definitions of citizenship and ethnicity are often highly politicized. In the case of Rwanda, ethnicity is indeed politicized, which is reflected in the omission of ethnic identity in the 2002 population census, in line with the public rhetoric of national unity after the 1994 genocide. But, leaving ethnicity aside, the Rwandan census data turns out to be very reliable. Comparing the 1991 Rwandan census data with 1990 population data from the local administration, Verpoorten (2005) finds an almost exact match of the total number of women and men, which is indicative for the quality of both sources, as they were collected independently of each other.

Another finding in support of the reliability of the Rwandan census is that, when replicating AdW with census data instead of DHS data, we find very similar estimates for the overall impact of conflict on schooling, as well as for the coefficients on the control variables.

AdW study years of schooling of a young (6-15) and an older (16-35) age cohort in the 1992 and 2000 DHS survey, and conclude that children exposed to armed conflict completed close to one-half year less education, corresponding to a 18.3% drop relative to the average educational achievement. For the replication of this result, we use the same empirical set-up, which consists of a difference-in-difference (DD) estimation. The young age cohort in 2000 represents the treatment group, as they were exposed to conflict at schooling age. The DD model can be written as follows:

$$Y_{ipt} = \alpha_0 + \alpha_1 (postwar_t * young_{ip}) + \alpha_2 postwar_t + \alpha_3 young_i + \varepsilon_{ipt} \quad (1)$$

where Y_{it} indicates the years of schooling of individual i living in province p at time t , *postwar* is an indicator variable for being in the post-conflict round, *young* is an indicator variable for being in the young age cohort and ε is the error term. A negative (positive) estimate of the DD coefficient, α_1 indicates that the young cohort in the post-conflict era completed less (more) years of schooling, compared to the young cohort in the pre-conflict era and relative to the difference in schooling between the old cohorts over the same period.

The DD model can be estimated using the 1991 and 2000 DHS data, which provide 18,314 and 27,086 observations, respectively, in the age cohort 6-35. Doing so, AdW find a DD estimator equal to -0.555, significant at the 1 percent level (column 1 of Table 2, below). In a narrow replication using DHS data we obtain a very similar point estimate of -0.504, significant at 1% (column 2).⁵

Using a 10% random draw of the 1991 and 2002 censuses, gives a much larger number of observations; 441,870 and 464,741, respectively, for the age cohort 6-35.⁶ Repeating the estimation using these data we obtain a point estimate of -0.573, corresponding to a 16.2% drop in years of schooling (column 3).⁷ This estimate is very close to the AdW result, despite slightly different sample years.

In the next step, we follow AdW by expanding equation (1) to include household level controls as well as province and age fixed effects:

⁵ Replication data and command files for AdW are not publicly available (yet). We therefore constructed the replication materials using the DHS survey and the information provided by AdW in their paper.

⁶ The 10% draw of the 1991 census is made available by Minnesota Population Center (2010). The same source also provides access to a 10% draw of the 2002 census, but this sample does not include the names of the administrative sectors. We therefore take a new 10% random draw directly from the full 2002 census at our disposal. Results remain qualitatively the same when considering the full census.

⁷ The proportional change in schooling is derived as the DD estimate when the dependent variable equals the natural logarithm of years of schooling.

$$Y_{itp} = \beta_0 + \beta_1 (postwar_t * young_{ip}) + \beta_2 postwar_t + \beta_3 young_{ip} + X_{ip}\Delta + \eta_i + \pi_p + \varepsilon_{itp} \quad (2)$$

where η_i and π_p represent the age and province fixed effects, respectively, and X is a vector that includes the same household level controls as AdW: an indicator variable for female, for non-poor households⁸, the age of the household head, the highest education level of any household member, the number of children under 5 living in the household, and an indicator variable for rural areas.

The DD estimate of the augmented empirical equation reported by AdW is -0.421, significant at 1% (column 4). In our narrow replication using DHS data we find a similar coefficient, equal to -0.494 (column 5). The broad replication using census data yields an estimate of -0.649 (column 6). In proportional terms, this latter figure amounts to a drop of 18.4%, which matches almost perfectly the 18.3% estimate of AdW, obtained through the DHS data.

Table 1 reports the summary statistics for the DHS and census variables used in the analysis.

---- Table 1 about here ----

Figure 1 visualizes the comparison between the two sources, giving the average years of schooling by age in both the pre- and post- conflict rounds for the DHS (panel A) and for the census (panel B) data. The figures are very similar. The old age cohorts display, on average, systematically higher years of schooling in the post-conflict era compared to the pre-conflict era, suggesting that before the explosion of the violence there was a positive trend in schooling in

⁸ The indicator variable *non-poor* indicates if the individual has more assets than the population mean. The assets include piped running water, refrigerator, radio, finished floor, bicycle, motorcycle, and car (AdW, 2011).

Rwanda. In contrast, and in line with the DD estimate, educational attainment of the younger cohorts is lower in the post-conflict era, compared to the pre-conflict era.

---- Figure 1 about here ----

In sum, the analysis presented in this section confirms the main result of AdW of a large drop in schooling and demonstrates that the DD estimates are very similar across the DHS and census data. In addition, as Table 2 clearly reveals, the estimated coefficients on the control variables are also similar. This is an indication for the reliability of the census data and lends support to their use in our exploration of the channels that account for the schooling deficit.

---- Table 2 about here ----

3. Leveling-off versus no leveling-off

We now shift our focus from exploring the overall effect of conflict on schooling to studying the heterogeneity of the effect across wealth and gender. To evaluate whether conflict affects years of education differently across the poor and non-poor and across boys and girls in the DHS samples, AdW rely on a Diff-in-Diff-in-Diff (DDD) strategy, adding a three-way interaction term to equation (2), in which the treated group (young age cohort in the 2000 DHS) is interacted either with a household-level indicator variable for being non-poor (*non-poor*young*postwar*) or with an individual-level indicator variable for being a female (*female*young*postwar*). The DDD coefficient on the triple interaction term should reveal whether the drop in schooling for the young cohort in the post-conflict era has been relatively larger or smaller for children in non-poor households and for girls. In order to obtain unbiased estimates, however, all the constitutive components of the triple interaction should be included in the regression. In their specification,

AdW left out some of these components, obtaining misleading estimates for the DDD coefficients.⁹

(a) Non-poor versus poor

Table 3 reports the results when the triple interaction with the *non-poor* variable is added to the regression. In their paper AdW find a large, negative and strongly significant estimate of -1.223 on the three-way interaction term (column 1), which leads them to conclude that the drop in schooling is stronger for the children in non-poor households. We can replicate this result almost exactly using the DHS data, provided that we omit the constitutive terms *non-poor*young* and *non-poor*postwar* (results not reported). When including these terms, however, we obtain an estimated coefficient of -0.399 (column 2), indicating a much smaller additional negative impact on schooling for the non-poor. Moreover, in our broad replication using the richer census data, the estimated DDD drops to -0.018 and is no longer significantly different from zero (column 3).

---- Table 3 about here ----

However, in addition to the exclusion of some constitutive terms and the limited representativeness of the DHS dataset, there is an endogeneity problem affecting these estimates, as poverty may itself be a determinant of conflict intensity or the non-poor may be characterized by different trends in pre-conflict schooling. To solve for the endogeneity one would need to employ a rich set of control variables and/or a valid instrument - both of which are in short

⁹ That all constitutive components of interaction terms need to be included is pointed out among others by Brambor et al. (2006), who write: "No matter what form the interaction term takes, all constitutive terms should be included. Thus, [...] X,Z, J, XZ, XJ, and ZJ should be included when the interaction term is XZJ. Although the statistics (and political science) literature is clear that all constitutive terms should be included, scholars often fall prey to the temptation to exclude one or more of them." (p. 66)

supply. We will therefore not be able to provide a final answer to whether the armed conflict disproportionately affected poor households in terms of years of education.

What we can do is highlight the need to qualify the results. Take for instance the different trends in schooling across urban and rural households, shown in Figure 2. Since the urban areas are home to many of the non-poor, these different trends are associated with different trends across poor and non-poor.¹⁰ When we run our regression including an indicator variable for urban communes¹¹ and the full set of interaction terms *urban*postwar*, *urban*young* and *urban*young*postwar*, the coefficient on the three-way interaction term *non-poor*young*postwar*, albeit of small magnitude, turns positive (column 4).

---- Figure 2 about here ----

In sum, the result of AdW of a higher schooling deficit for non-poor fades away when adding the constitutive components of the interaction terms and using a more representative sample; and it is reverted when accounting for part of the non-randomness of poverty by including a set of control variables for urban trends.

(b) Boys versus girls

Conflict cannot turn boys into girls, or girls into boys; and boys and girls are not clustered in space. This makes it easier to study the heterogeneous impact of conflict across gender, as the three-way interaction term *female*young*postwar* can be interpreted unambiguously.

Table 4 reports the results when the triple interaction with the *female* dummy is added to the regression. In this case AdW report a positive and strongly significant (at 1%) DDD estimate of

¹⁰ Using the measure of poverty constructed by AdW, we find that 42% of people living in rural areas were non-poor in 1991, compared to 81% of people living in urban areas.

¹¹ The urban indicator variable takes the value one for the communes of Kigali City, the provincial capitals and a number of other secondary urban centers.

0.219, suggesting that the impact of the conflict on the schooling of girls is less negative than the impact for boys, thus pointing to a leveling-off effect (as girls have on average a lower level of schooling).

---- Table 4 ----

However, also in this case, two out of the three constitutive terms of the interaction effect are erroneously omitted (*female*young* and *female*postwar*). In a narrow replication on the basis of the DHS data and omitting the constitutive terms of the DDD we also find a positive and strongly significant coefficient (0.148, not reported). In a correct specification we estimate instead a non-significant coefficient of -0.039 and, when using the richer census data, we obtain a significantly negative coefficient (at 1%) equal to -0.141, thus indicating an additional schooling deficit for girls.

To be sure, we run the regressions on subsamples, for boys and girls separately. We find a DD estimate of -0.595 for boys and -0.705 for girls; indicating that the conflict led to a drop in schooling of 15.9% for boys, compared to a drop of 20.7% for girls (not reported). The result is very robust to changes in the age thresholds considered, but the magnitude of the coefficient decreases as we include older individuals in the young age cohort. This suggests that the additional negative effect of conflict on girls' schooling is stronger for the youngest cohorts, but fades away when considering older individuals. This is confirmed by Figure A1 (in appendix), in which we report the estimated DDD coefficient for each age, from 6 to 20. Exploring the reasons underlying this pattern goes beyond the scope of this paper, but this is a result that deserves further research.

As the DDD estimate is unlikely to be affected by confounding factors, it should remain stable when adding the urban factors (*urban*postwar*, *urban*young* and *urban*young*postwar*).

We indeed find that when adding these terms, the estimated DDD coefficient is virtually unchanged, switching from -0.141 to -0.149.

In sum, the result of AdW of a higher schooling deficit for boys is reverted when adding the constitutive components of the interaction terms. Our findings point to an additional negative conflict impact on the schooling of girls.

4. Alternative age cohorts and the drop in secondary schooling

So far, we have considered the age group 16-35 as a control group for studying the impact of conflict on the young cohort 6-15. Doing so, we follow the baseline specification of AdW. There are however two issues related to this approach.

First, strictly speaking, the age groups used in our broad replication are not a one-to-one match with those of AdW. One could argue that a closer match to the age cohort used in AdW is the age cohort 8-37 in the 2002 census, since individuals aged 8-37 in 2002 were aged 6-35 in 2000, the DHS survey year. We therefore repeat our broad replication of the main result obtained by AdW (and reported in Table 2) using the age cohort 8-17 as the young cohort and 18-37 as the old cohort. The results, shown in Table 5, are qualitatively the same to our baseline specification (reported in column 1), but quantitatively larger, with a DD estimate of -0.919 for equation (2), corresponding to a 21.1% drop in years of schooling.

---- Table 5 about here ----

The larger coefficient obtained with this “older young cohort” hints to the second issue related to the cut-off ages, i.e. the old age cohorts considered so far are imperfect control groups, as they include individuals that were still at schooling age when the genocide broke out. For instance, individuals aged 16 to 22 in 2002 were aged 8 to 14 in 1994. Their schooling

attainment may therefore have been affected by the conflict.¹² This is clear from Figure 1, which shows that, on average, years of schooling in 2002 are lower than in 1991 for individuals up to 22 years old. When repeating our estimation using the expanded young age cohort 6 to 22 (and contracted old age cohort 23-35), we find a larger drop in years of schooling of -1.176, corresponding to a proportional decrease of 24.8% (Column 3 of Table 5).

The expanded young age cohort, 6-22, includes individuals at both primary and secondary schooling age. To pin down the different effect of conflict across primary and secondary school, we run two separate regressions for individuals at primary and secondary schooling age. To demarcate primary and secondary schooling age, we rely on information from the 1991 census, displayed in Figure 3. The figure shows that most students up to the age of 13 are attending primary school, while from age 14 onwards most students attend secondary school. We therefore use the young cohort 6-13 to approximate the impact on primary schooling (excluding the age group 14-22), and we use the young cohort 14-22 to approximate the impact on secondary schooling (excluding the age group 6-13).¹³ In both cases, 23-35 acts as a control group. Column 4 and 5 of Table 5 show that we obtain a DD estimate of -1.104 and -1.073 respectively, corresponding to a drop of 27.5% in the years of primary schooling and a drop of 17.4% in the years of secondary schooling.

---- Figure 3 about here ----

In sum, this section indicates that the overall drop in primary schooling caused by the armed conflict is likely to be considerably larger than originally thought, increasing from 18.4% to 27.5% when the cut-off ages are adjusted to take into account that children at secondary

¹² According to the 1991 census, 39% of the children aged 8 to 14 were enrolled in school in Rwanda.

¹³ In 1991, 99% of the students were under 23; the age group 6-13 represented more than 92% of the student population enrolled in primary school, while the age group 14-22 represented 73% of the student population enrolled in secondary school.

schooling age also suffered a drop in schooling due to the conflicts. We also find that the drop in secondary schooling has been large, although not as large as the drop in primary schooling.

Even though these results indicate that the age cohort 16-35 is not the best choice as a control group, we will keep using so to allow direct comparison with AdW. Given this choice, our estimated effect of conflict on schooling attainments should be considered as a lower bound. In a series of robustness checks we re-run all our regressions using the cohorts 8-17 and 6-22 as young age cohorts. We find that all our results - including those presented in the previous chapter, concerning the leveling-off hypothesis - remain in a qualitative sense, except in few cases, which we will highlight in the text.

5. School initiation, drop-out or slow grade progression?

The drop in schooling attainment can be due to children that do not initiate school (H1), children that drop out of school (H2), or children that slowly progress through grades (H3).¹⁴ We investigate the relative importance of these channels on the basis of information on the past and present schooling status of the individuals in the census, i.e. whether he or she has ever attended school (*everbeentoschool*) and whether he or she still is a student (*student*).¹⁵

We first produce a set of simple figures. Figure 4a displays the shares of individuals who ever attended school by age and year. Compared to 1991, individuals of all ages in 2002 were

¹⁴ AdW investigate the role of school initiation (H1) by studying grade completion separately for grades 1 to 6. They find that first-grade completion is very similar across 1992 and 2000 and conclude that the estimated schooling deficit is not so much caused by a lack of access to education, but rather by difficulties to continue attending school and to progress through grades. However, AdW do not empirically explore the relative contribution of dropouts (H2) and grade repetition (H3).

¹⁵ More precisely, census data record the years of schooling a person has completed and whether he or she is currently studying. We therefore assume that a person has been to primary school if he or she is currently a student or, alternatively, if he or she has completed at least one year of school.

more likely to have ever attended school, indicating that primary school initiation (H1) is not a major issue. Figure 4b gives information on dropouts, displaying the share of individuals aged 14 or above that completed all six grades of primary school, among the population that started primary school but that is no longer enrolled there. The figure shows that, in the census year after the conflict, more students dropped out before completing primary education, providing support for (H2). Finally, if slow grade progression (H3) is among the causes of the observed drop in schooling, we should find that students in 2002 are older than students in 1991, conditional on the grade they attend. This is shown in Figure 4c, which gives the average age of students across twelve different grades. The average age of students is higher in 2002 than in 1991 for each of the six primary grades (1-6) and across the three years of lower secondary schooling (7-9), but not for upper secondary schooling (grades 10-12). For instance, students who attend the final grade of primary school (i.e. who already completed 5 years of schooling) are on average 14.7 years old in 2002 compared to 12.5 in 1991.

---- Figure 4 about here ----

Combined, these figures suggest that drop-outs (H2) as well as grade repetition (H3) are driving the result of a decrease in primary schooling, while primary school initiation (H1) is not a major issue. We study this more formally.

First, focusing on school initiation (H1), we repeat the estimation of equation (2) when replacing years of schooling with *everbeentoschool* as a dependent variable.¹⁶ The first column of Panel A of Table 6 shows that the estimated coefficient on the interaction term *young*T* is negative and significant, but small, indicating that the young cohort in 2002 was 2.3 percentage

¹⁶ Although this variable is binary, we keep the linear estimation model, because it is more traceable and any possible bias will be small given the large number of observations. Results are confirmed when using a Probit model (not reported).

points less likely to enroll in primary school, compared to the older cohort in 2002 and relative to the difference between the young and old cohort in 1991 (the average enrolment rate in the sample is 68.2%).¹⁷

To study whether dropouts (H2) played a role, we take primary school completion as our dependent variable. Focusing on those individuals that ever attended school, but are no longer enrolled in primary school, we redefine the young cohort as those aged 14 to 22 and the old cohort as those aged 23-35.¹⁸ The estimate reported in the second column of Panel A of Table 6 is again negative and significant, but this time of sizeable magnitude: in 2002, former students in the young age cohort were 28.1 percentage points less likely to have completed primary school, compared to the older cohort in 2002 and relative to the difference between the young and old cohort in 1991 (average primary completion rate in the sample is 54.8%).

---- Table 6 about here ----

To study the case of slow progress (H3), we restrict our analysis to individuals enrolled in grades 1-6, and to the age group 10-18 because information on student status is available only from age 10 onwards, and because above age 18 there are very few primary school students.¹⁹ We introduce an individual's age as the dependent variable in the following equation:

$$A_{itg} = \delta_0 + \delta_1 postwar_t + X_i \Psi + \Gamma_g + \pi'_p + \mu_{itp} \quad (3)$$

where the terms A_{itg} and Γ_g indicate the age of child i at time t in grade g , and grade fixed effects, respectively. The coefficient of interest is δ_1 . It indicates whether, for a given grade, students are on average older in the post-war population than in the pre-war population. Our estimated

¹⁷ When using the alternative young age cohorts 8-17 the estimated coefficient is statistically insignificant.

¹⁸ According to the 1991 census, 93% of the students enrolled in primary school were younger than 14. Results are in any case very robust to small changes in the definition of the cohorts.

¹⁹ According to the census rounds, the share of individuals over 18 attending primary school is less than 0.5% in 1991 and less than 2% in 2002. Once again, results are very robust to small changes in the age thresholds.

coefficient of 1.694 provides clear evidence for slow grade progression: on average students in 2002 were over one and a half years older than students in the same grade in 1991.

Panel B of Table 6 shows the results for secondary schooling. In sharp contrast to primary schooling, but in line with the patterns in Figures 4a, 4b and 4c, the results indicate that school initiation is the most salient factor explaining the drop in secondary schooling.²⁰ In column 1 only individuals that completed primary school are considered. The coefficient indicates that, in 2002, individuals in the age cohort 14-22²¹ were 28.5 percentage points less likely to complete the first year of secondary school compared to the same cohort in 1991 (average secondary school enrolment in the sample is 59.7%). On a positive note, results in the second column indicate that those who started secondary schooling were not less likely to finish it (if anything, they were more likely to do so), and this despite evidence for a slightly slower grade progression (column 3).²² One possible explanation for this result is that the selection of students that did enroll into secondary school after the conflict was biased towards the relatively more motivated and able.

In order to test whether these channels operate similarly for boys and girls, we add to our regression the interactions with the *female* dummy. Results reported in Table A1 in Appendix reveal that our previous conclusions on the relative importance of the three factors hold for both boys and girls: slow grade progression and high dropout rates are the major factors leading to the drop in primary schooling, while a lower enrolment drives the result for secondary schooling.

²⁰ As the census only records the years of schooling completed by each individual, along with his or her current student status, we define our enrolment variable as taking value 1 if either the person has completed at least one year of secondary school, or if he or she has completed primary schooling and is currently a student.

²¹ See Section 3 for a justification of the choice of the 14-22 age category.

²² We choose 18 as the lower limit because according to both the 1991 and 2002 census virtually nobody completed secondary schooling before that age. The choice of the upper limit of 26 is more arbitrary. In any case, less than 0.5% of individuals enrolled in secondary school in 1991 were above 26. Results are very robust to small changes in the definition of the upper limit.

However, although the overall picture holds, there are some noteworthy differences. First, the (small) drop in primary school enrolment that we observed above is mostly driven by a drop in the enrolment of girls. Second, compared to boys, girls suffer a significantly higher dropout rate during primary school. There is however no indication of slower grade progression for girls. Moreover, conditional on completing primary school, girls are somewhat more likely to enroll in secondary school than boys, even if they then suffer a slightly higher dropout rate from secondary school.

6. Attribution to genocide: contrasting findings

Among the events in Rwanda's conflict cycle of the nineties - civil war, genocide, revenge killings, (counter-)insurgency operations and a massive refugee crisis - the genocide stood out with a death toll of close to 800,000 in barely 100 days (between April and July 1994). AdW test whether the drop in schooling can be attributed to the genocide by estimating a DDD model in which the treatment group (young age cohort in the post-war round) is interacted with a measure of genocide intensity. In other words, they augment equation (2) with the term *postwar*young*genocide_intensity*, this time including all its constitutive components.

AdW consider three different genocide intensity measures defined at the level of 11 provinces: the proportion of days during which killings occurred in a province in the months April-June 1994 (Measure A), an indicator variable taking one for the three provinces with the highest number of killings in 1994 (Measure B), and the number of mass graves and memorials

per province (Measure C).²³ The DDD estimates obtained by AdW – reported in Panel A of Table 7 - are all negative and significant at the 10% level, pointing to a significantly stronger negative impact on schooling in provinces where the genocide intensity was higher.

We cannot replicate these results. As Panel B of Table 7 shows, in our narrow replication, using the same data, variables and specification, we obtain very different coefficients. It is not clear what accounts for these differences.²⁴ Estimates are also very different when we estimate the DDD model using the population census data, with the coefficients turning all close to zero (Panel C).

---- Table 7 about here ----

In an attempt to address the potential endogeneity of genocide intensity, AdW perform an IV estimation. Using the province-level distance to the Ugandan border as an instrument, AdW report IV coefficients that are in line with their OLS results. Once again, we obtain very different results – mostly not statistically significant - both in our narrow and in our broad replication.²⁵

Leaving aside the comparison with the results of AdW, our failure to establish a link between the drop in schooling and the genocide may be due to the fact that the conflict intensity measures considered so far are all defined at the province level and may be too crude to properly capture variation in genocide intensity. We address this issue by turning to four finer measures, defined at the level of the 145 administrative communes: the proportion of Tutsi in the pre-war

²³ Measure A and B are taken from the genodynamics project (Davenport and Stam, 2009); measure C is taken from the Yale Genocide Studies website (<http://www.yale.edu/gsp/rwanda/>)

²⁴ Dropping the province fixed effects, or the constitutive components - one by one, or in pairs – does not bring the replication results closer to the results presented by AdW.

²⁵ Results are available on request. We do not discuss them in detail because the exclusion restriction is unlikely to hold. To defend the choice for the instrument, AdW argue that the southern provinces located further from the border with Uganda were more likely to experience high conflict intensity because they were reached later by the Rwandan Patriotic Front, which brought an end to the killings of Tutsi. Although this instrument is relevant, i.e. it is sufficiently correlated with 1994 killings, it is unlikely to be exogenous, as the southern part of the country was also characterized by a higher concentration of Tutsi, which traditionally enjoyed higher levels of education (see also our discussion in Section 7).

population, the share of genocide suspects, the number of mass graves, and the distance to the nearest mass grave calculated from the commune centroid.²⁶ Table 8 reports the estimates: none of the measures yields a DDD that is significantly different from zero and of the expected negative sign.²⁷

---- Table 8 about here ----

In sum, the DDD model does not attribute the observed drop in schooling to the genocide, even when using finer genocide intensity measure and a larger and more representative dataset. This is also confirmed in a subsample analysis. The last two rows of Table 8 report the estimated DD coefficient when equation (2) is run on a restricted sample, considering only communes that have a value above or below the median for each one of the four different conflict variables. The estimated DD coefficients are always very similar across the two subsamples.

The mismatch between the drop in schooling and genocide intensity is also clear from Figure 5, in which we compare the distribution of the commune-level DD estimates obtained by estimating equation 1 for each commune j separately (figure 5a) to the distribution of the share of Tutsi living in each commune in 1991 (figure 5b). For instance, since the share of Tutsi in the northwestern provinces was as low as 1.5% (compared to over 10% in the South), it is unlikely that the large estimated drop in schooling in the Northwest is due to the genocide. The spatial mismatch also holds for each of the other conflict proxies (see Figure A2 in Appendix).

---- Figure 5 about here ----

²⁶ The first of these measures is calculated from the 1991 Rwandan population census, the second from the records of the Gacaca (the transitional justice system for genocide suspects), and the latter two from a map taken from the Yale Genocide Studies website.

²⁷ We also repeat our regressions instrumenting the conflict variables (and their interactions) with the commune-level (log) distance to the border with Uganda. The qualitative results remain the same (not reported). Finally, we repeat the regression including commune fixed effects to limit the likelihood of bias stemming from omitted time-invariant factors, and find again similar results (not reported).

7. Why can't we attribute the drop in schooling to genocide?

7.1. *A more general regional or national time trend*

One possible explanation for the mismatch is that the drop in schooling picked up by the DD model is unrelated to genocide and is instead simply driven by a time trend. In order to test for this possibility AdW compare the trends in schooling in Rwanda in the nineties with those in neighboring countries (Kenya, Tanzania and Uganda). They demonstrate that years of schooling consistently increased for the Rwandan neighbors, indicating that the observed drop in Rwanda cannot be accounted for by regional events or regional time trends.

We performe another falsification test to verify whether a longstanding national time trend is driving our results. We estimate the DD model for two older cohorts of individuals, unlikely to be exposed to armed conflict at schooling age, i.e. 25-40 and 41-60. The DD coefficient, estimated at -0.017, is not significantly different from zero (not reported), indicating that our results cannot be due to a pre-existing time trend in education.

7.2. *Confounding factors*

Could the failure to establish a direct link between the drop in schooling and the intensity of the genocide be due to the presence of confounding factors that bias our DDD estimates? We can think of four such factors: pre-war regional trends in schooling, migration, selective killings during the genocide and post-war assistance to genocide survivors.²⁸

²⁸ Once again, for ease of exposition, in what follows we only discuss results in which we consider the share of Tutsi living in the commune in 1991 as the proxy for genocide intensity, but results are similar whenever one of the alternative proxies is considered (not reported, but available on request).

(a) Pre-war regional time trends in schooling

The DDD takes the pre-war trend across young and old as a counterfactual, implicitly assuming that conflict intensity does not correlate with pre-war trends in schooling. This assumption is violated if both conflict and schooling trends are affected by a common factor. One such factor may be the identity of the political leader.²⁹ In Rwanda, prior to independence, the Tutsi elite was dominating and the areas around the capital of the Tutsi monarchy (Nyanza, located on the intersection of Gitarama and Butare) were flourishing. The ethnicity of the leader changed from Tutsi to Hutu in 1959, and discrimination against Tutsi increased, especially during the Habyarimana regime (1973-1994), when favors were directed to the core of Habyarimana's supporters, residing in the provinces of Gisenyi and Ruhengeri, in the Northwest of the country (Des Forges, 1999).³⁰

Clientelism and switching powers may therefore account for pre-war differences in schooling trends across the country.³¹ To check whether these pre-war trends are confounding our DDD results, we exploit the large size of our dataset and add a complete set of province trends and province interaction terms to the DDD regression model (i.e. for each province p we create the indicator variable $province_p$ and we add the terms $province_p*young*postwar$, $province_p*young$, $province_p*postwar$ to the regression). Panel A of Table 9 reports the results: the DDD estimate remains statistically indistinguishable from zero.

²⁹ Kudamatsu (2009) and Franck and Rainer (2012), among others, show that there is a strong link between the identity of political leaders and the development of the regions that are favored by them.

³⁰ About Habyarimana's political party, the Mouvement Révolutionnaire Nationale pour le Développement (MRND) Des Forges (1999, p.45) writes: "...the MRND had regulated access to government-supported high schools, supposedly assigning places according to quotas for ethnic and regional groups. The quotas were both inaccurately computed and unfairly applied, favoring children from the Northwest or those whose families could pay in money or other benefits for access to education."

³¹ Figure A3 in Appendix shows that whereas individuals in the old age cohort in Butare and Gitarama had higher schooling in 1991 than their counterparts in Gisenyi and Ruhengeri, the difference is minimal for the young cohort in 1991. This pattern is consistent with a catch-up of schooling in the stronghold of the Habyarimana regime. The pattern remains even when removing Tutsi from the sample, suggesting that - besides ethnicity - regionalism played a role.

(b) Migration

Armed conflicts are often associated with large migration flows, both during the conflict and in its aftermath. If especially highly educated adults moved out of the most affected communes, the gap between the young and old cohort remaining in those communes would be reduced, thus biasing our DDD estimate towards zero.³²

The census data include information on place of birth, previous residence, current residence and time at current residence, allowing us to trace an individual's migration history. To gauge whether migration is causing a bias we assign all individuals who moved between 1994 and 2002 - representing 21.7% of the 2002 sample - to their previous commune of residence and we re-estimate the DDD model.³³ The results, reported in Panel B of Table 9, show that the DDD coefficient remains (positive and) statistically insignificant.³⁴

---- Table 9 about here ----

(c) Selective killings

de Walque and Verwimp (2009) demonstrate that the probability of being killed in the genocide was relatively higher for men, for the well-educated and for Tutsi, and was highest among the well-educated Tutsi male population. Figure 6 shows that Tutsi had on average more years of

³² Figure A4 in Appendix shows that adults who migrated between 1994 and 2002 had on average higher levels of education compared to non-migrants.

³³ The sample size is reduced because individuals that previously lived abroad are dropped from the sample. It is also important to mention that this test is complicated by the fact that in 2001 Rwanda underwent a large administrative reform that transformed its 145 communes into 106 districts. The 2002 census only provides previous district of residence. For those districts that overlap with different communes we arbitrarily assign the individual to one of the communes. Results are however robust to alternative assignments to the previous communes, to dropping all migrants, and to only re-assigning migrants from one of the 20 districts that perfectly match with previous communes (results not reported, but available on request).

³⁴ We cannot account for individuals who migrated outside Rwanda after the genocide and did not return, as they are not included in the 2002 census. The magnitude of the potential bias introduced by these non-returnees is however small. As it will be better explained in chapter 8, the large majority of individuals that fled Rwanda in 1994 were refugees that sought repair in the refugee camps set up in the neighboring countries, which closed in the course of 1996-1998 generating a massive flow of returnees, as reported, among others, by the Red Cross (Merkelbach, 2000).

schooling than Hutu in 1991. The targeted killing of Tutsi adults would thus bias downward our DD and DDD estimates, since it would reduce the gap between the schooling of the young and old age cohorts in the post-war population, and especially so in the provinces and communes with high genocide intensity.

---- Figure 6 about here ----

As it has been estimated that approximately 75% of Tutsi were killed during the genocide, we gauge the magnitude of the bias by re-running our estimations after randomly removing 75% of Tutsi from the 1991 population, i.e. after artificially introducing in the 1991 census a selection similar to the one caused by the genocide in 1994. This artificial manipulation of the sample, which results in the loss of around 3% of the overall observations, pushes the DDD coefficient down to -0.042, but leaves it statistically insignificant (Panel C of Table 9). Moreover, the large and significant coefficient of the two-way interaction term (*young*postwar*) remains, indicating that the localized effects of the genocide cannot fully account for the drop in schooling, even when controlling for the selective aspect of the killings.³⁵

(d) Post-war assistance to genocide survivors

In the aftermath of the conflict, many assistance programs for genocide survivors were launched. For instance, the FARG (Fonds d'Assistance aux Rescapés du Génocide) supports genocide survivors with allowances, health insurance, housing and school fees, among others. According to the information provided on the FARG website (www.farg.gov.rw), four years after the

³⁵ A stronger test would be to drop the 75% best educated Tutsi from the old age cohort, instead of a random sample. Doing so results in a negative and significant (at 1%) DDD coefficient of -0.696 (not reported), indicating that the selective aspect of the killings has the potential to mask the link between genocide intensity and the drop in schooling. However, it still cannot account for the large overall drop in schooling, because the DDD remains quantitatively small (the average share of Tutsi in the sample is equal to 0.11) and, above all, because the large and significant coefficient of the two-way interaction term (*young*postwar*) remains virtually unaffected.

genocide FARG was already awarding scholarships for secondary schooling to 24,000 students, which is a sizeable share of genocide survivors at schooling age.³⁶ Even in the absence of detailed data on the other forms of support, this figures is suggestive of the large amount of resources mobilized to support genocide survivors.³⁷

The support received by genocide survivors may confound the link between the intensity of the genocide and the drop in schooling. Figure 7 illustrates why this could be the case. Relying on data from a nationally representative survey collected in 1999/2000 (EICV1), the figure shows a markedly positive relationship between the share of students that report enjoying a scholarship in 2000 and the province-level share of Tutsi in the pre-war population.³⁸ The strong link between the scholarship program and the genocide is also underscored by the fact that most of its beneficiaries (63%) are paternal orphans.³⁹

---- Figure 7 about here ----

But, is the scholarship program important enough to confound our DDD result? It can bias the DDD estimate to zero if it sufficiently drives up years of education of the young cohort. We cannot directly test this because the census does not provide information on scholarships, but we can get an idea about the maximum possible bias induced by the scholarship program by removing from the young age cohort of each province a share of the best educated children, equal to the province-level share of children that received a scholarship according to the EICV1

³⁶ It has been estimated that in total 300,000 Tutsi survived (Prunier, 1998) – among which about 20% would have been at secondary schooling age in 1998.

³⁷ Although the scholarship program of the FARG targets secondary schooling, the foresight of access to secondary schooling may positively affect the decision to send children to primary school. Besides, primary schooling may be funded by the cash allowances, which amounted to RWF 5000 monthly, and were received by about 30,000 people in 2011 (www.farg.gov.rw).

³⁸ The relationship holds for students in different age groups, e.g. 6-35, 6-15 and 16-35.

³⁹ The channeling of scholarships to genocide orphans could account for an apparently surprising finding in the data, i.e. the smaller difference in schooling between orphans and non-orphans in provinces with a high share of Tutsi, compared to provinces with a low shares of Tutsi (see Figure A5 in Appendix).

data. For instance, since 14% of students are reported to receive a scholarship in Gitarama province, compared to 5% in Gisenyi province, we drop from our sample 14% of the best educated children in the young cohort in Gitarama province and 5% in Gisenyi province. Doing so for each province, we find that the DDD coefficient declines to -0.230, but remains statistically insignificant (Panel D of Table 9). We can therefore conclude that the potential bias induced by the program cannot account for the statistical mismatch between genocide intensity and the drop in schooling.⁴⁰

8. The puzzle remains: what accounts for the large drop in schooling?

When accounting for potentially confounding factors, the DDD estimate changes, but remains statistically indistinguishable from zero. Moreover, the coefficient of the two-way interaction term *young*postwar* remains remarkably stable across all the tests we performed. The puzzle thus remains: we cannot attribute the large drop in schooling to the localized effect of the genocide. In this section we dig deeper into this puzzle, investigating whether the drop in schooling is driven by other specific forms of violence that affected Rwanda in the nineties. After excluding this possibility, we shift our attention to the relationship between the drop in schooling and external displacement. In this case we find that the drop in schooling can be at least partly explained by the fact that a high number of individuals abandoned Rwanda to seek temporary refuge in one of the many camps set up in the neighboring Democratic Republic of Congo (DRC), Tanzania or Burundi. Importantly, the spatial distribution of the externally

⁴⁰ Results are robust to excluding from the analysis all paternal orphans. The DDD coefficient only turns negative and significant when considering the young age cohort 6-22. However, also in that case the coefficient of the interaction *young*postwar* remains very stable, large, negative and significant at 1%.

displacement households is not correlated with any specific form of violence. Hence, in the concluding section we argue that the impact of armed conflict in Rwanda was nationwide; and we provide additional pieces of evidence in support of this conjecture.

(a) Other forms of violence

The 1994 genocide was extremely violent (around 800.000 victims), but short-lived (around 100 days). Other less intense, but longer-lasting forms of violence took place in Rwanda during the nineties, including civil war, revenge killings, insurgency and counter-insurgency operations. Could it be that the drop in schooling picked up by the DD model is driven by these other events? To test this hypothesis, we rely on the scarcely available data on other forms of violence in Rwanda.

The first dataset is taken from Serneels and Verpoorten (2013) and contains an index that records excess mortality caused by episodes of violence different from the genocide. The index was generated by relying on eleven excess mortality and conflict proxies calculated from the two waves of census data and the gacaca information round.⁴¹ The second dataset is based on the information contained in four Amnesty International Reports (1996, 1997a, 1997b, 1998) and records the number of extrajudicial killings of civilians during the (counter)insurgency operations that took place till the late nineties in the Northwest of Rwanda.⁴² While the first dataset has national coverage, the second dataset only contains information for the two provinces in the Northwest.

Panel A and B of Table 10 report the results using these two alternative conflict proxies. The DDD estimates are negative, and in general statistically insignificant, turning significant only for

⁴¹ Principal component analysis was used to construct the indices. See Serneels and Verpoorten (2013) for details.

⁴² Verpoorten (2012) provides details on the compilation of this dataset.

operations that took place till the late nineties in the Northwest of Rwanda.⁴³ While the first dataset has national coverage, the second dataset only contains information for the two provinces in the Northwest.

Panel A and B of Table 10 report the results using these two alternative conflict proxies. The DDD estimates are negative, and in general statistically insignificant, turning significant only for certain cut-off ages.⁴⁴ These results suggest that other forms of violence may *partly* account for the drop in schooling. However, the fact that the *young*postwar* coefficient remains negative, large and significant at 1%, indicates that the large drop in schooling remains mostly unexplained, also by other specific forms of violence.

---- Table 10 about here ----

To further confirm this finding, we calculate DD estimates of the drop in schooling for each province and compare these with information on the type of violence (genocide, civil war or (counter-)insurgency) that dominated in each one of the 11 provinces during the nineties.⁴⁵ Table 11 shows that the DD coefficients are remarkably similar across the different provinces, showing no relationship between the drop in schooling and the predominance of any specific form of violence.

---- Table 11 about here ----

(b) Displacement

Amidst the climate of fear caused by the genocide and the advancement of the Rwandan Patriotic Front in 1994, almost 2 million people - about 25% of the Rwandan population - sought refuge in

⁴³ Verpoorten (2012) provides details on the compilation of this dataset.

⁴⁴ The coefficient of the Excess Mortality Index turns significant when the alternative age cohorts 8-17 or 6-22 are considered.

⁴⁵ This information is taken from Justino and Verwimp (2008), who constructed their index based on event data from various sources.

the case we run again the DDD model, but we now interact our treatment group with the share of externally displaced households living in a commune in 2002. While the 2002 census records detailed information on migration, it does not specifically record past refugee status and we therefore have to resort to a proxy. In particular, we consider the members of a household as having been externally displaced if in that household at least one child was born in DRC, Tanzania or Burundi between 1994 and 1998. Overall, 87% of the household in 2002 has at least one child born in that period and for 37% of these households the child was born in DRC, Tanzania or Burundi. We compute this share for each commune and we use it as our proxy for the share of displaced households living in the commune.⁴⁵ Importantly, this share is not correlated with any conflict intensity proxy, indicating that communes that experienced higher conflict intensity were not more (or less) likely to host externally displaced individuals in the aftermath of the violence.⁴⁶

Panel C of Table 10 reports the results. The DDD coefficient is negative and significant at 1%, indicating that the drop in schooling was indeed more severe in communes with a higher share of displaced households. This result is confirmed when we split the sample between communes with above-the-median versus below-the-median shares of displaced households and we run the DD model separately: in the former case the absolute value of the coefficient (-0.715) is significantly larger than in the latter case (-0.550), as reported in the row at the bottom of the table. At the same time, however, the coefficient of the double interaction *young*postwar*

⁴⁵ In order to make sure we are truly capturing displaced Rwandan households and not simply immigrants, we exclude from the computation those households that, in addition to a child born abroad, also have an adult member that was born abroad.

⁴⁶ The highest correlation is equal to 20% and is between the displacement proxy (defined at the household level) and the non-genocide excess mortality proxy. We also performed a more rigorous test, regressing the displacement proxy over each of the different conflict intensity proxies, including province fixed effects and clustering standard errors at the commune level. The estimated coefficients of the conflict variables are all close to zero and none of them is statistically significant.

remains negative, large and significant at 1%, indicating that external displacement cannot fully explain the observed drop in schooling.

(c) The cycle of violent events in the 1990s had a nationwide impact

So far we have shown that Rwanda experienced a large drop in schooling during the nineties. We have excluded the possibility that this drop is part of a more general time trend. We have also excluded our first best guess, i.e. that the drop directly relates to the localized effects of the genocide, and we have shown that it can neither solely be attributed to other specific forms of violence that took place in the nineties. Finally, the results in the previous section indicated that the drop is partly driven by the refugee crisis.

Putting these pieces of evidence together leads us to conjecture that the impact of the genocide on schooling was nationwide, because its disruptive effect was nationwide, because it operated through national channels, and because the genocide coincided with other forms of violence and events leaving no single area in the small country unaffected. There is a basis for this "residual" explanation.

First of all, as mentioned in the previous section, the data show a very low correlation between the location of origin or resettlement of the externally displaced individuals and any conflict intensity measure, indicating that the (indirect) impact of the conflict was felt well beyond the conflict hotspots.

Second, a 1994 report from the Ministry of Education (MINEPRISEC/MINESUPRES, 1994) indicates that the education system was severely affected with as many as 65% of the 1836 schools reported to be damaged and needing urgent repair. Importantly, school buildings across the genocide-affected and other communities were equally likely to have incurred damages. We

test this by relying on a nationally representative community survey collected in 1999/2000 (EICV1). The data indicate that communities more affected by the genocide were equally likely to be in the proximity of an operating primary school, and not more likely to have been the location of reconstruction or renovation works of a school building since 1994.⁴⁷

Third, besides school infrastructure, teachers had become a very scarce resource all over the country: many were killed, because they were Tutsi or part of the moderate Hutu elite; several others were imprisoned (having participated in the killings); and still others had moved abroad or to urban centers (Obura, 2003). The result was “*the total erosion of faith in the education system*”, with less than half of qualified teachers remaining in the primary system after the conclusion of the conflict (*ibidem*, p.48). Importantly, in the 2002 census, there is no relationship between the share of teachers and genocide intensity in a commune, suggesting once again that the disruption of the schooling system was not limited to the areas where the genocide was more intense, but spilled over also to the other regions of the (small) country.⁴⁸

Finally, the conflicts of the nineties heavily impacted the government budget, drying up resources for education. While the post-conflict era witnessed an increase in resources dedicated to the reconstruction of schools and infrastructures, by 2001 current spending to support the day-to-day running of the schooling system was still as low as 3% of the GDP, the same level that the country had in the eighties (World Bank, 2004).

⁴⁷ Results are based on regressions in which the dependent variable is the answer to one of the following questions: “*Is there a primary school in the village?*”, “*What is the distance to the closest primary school?*” and “*Is there a school built after the 4th of July 1994 in the cell?*”. The regressors include a measure of genocide intensity (results are robust to using any measure) and province fixed effects. Results are not reported, but available on request.

⁴⁸ In order to test for this, we use information of the sector of employment for each individual from the 2002 census and run a simple regression of the share of teachers over the conflict intensity proxy (plus province fixed effects). Unfortunately the 1991 and 2002 census rounds use different classifications for sector of employment. When we try to make them comparable by generating a variable that groups together all education-related jobs (for 1991: “teaching – general” and “teaching - technical and vocational”; for 2002: “primary teaching”, “secondary teaching” “advanced teaching” and “permanent education”), we find a Diff-in-Diff coefficient *postwar*genocide_intensity* not significantly different from zero.

9. Conclusion

By now, a large number of studies have documented the impact of armed conflict on schooling. Our study provides several contributions to this literature.

First, resorting to an improved empirical strategy and a more representative dataset, our broad replication of AdW yields different and (we argue) more correct results. This not only suggests that replication of existing empirical work is not a futile exercise and should be encouraged, but it also indicates that survey data that is not representative at the levels of small administrative units or other subgroups of the population may not be appropriate for pinning down the heterogeneity of the conflict impact.

Second, while previous studies have focused solely on primary schooling, our large dataset allowed us to estimate the impact of conflict on both primary and secondary schooling. We demonstrated a large drop in secondary schooling (17.4%). Furthermore, we showed how this drop has implications for the estimation of the drop in primary schooling. In particular, the estimated drop increased considerably when excluding from the control group children who were at secondary schooling age at the time of the conflicts. Given this finding, it is possible that studies that ignored the impact on secondary schooling, have underestimated the impact on primary schooling.

Third, we provided novel evidence on the relative importance of the channels underlying the drop in schooling, i.e. school initiation, slow grade progression and dropouts. We showed that dropouts and slow grade progression mostly account for the drop in primary schooling, while school initiation is the main factor explaining the drop in secondary schooling. The finding that a sizeable share of the observed decline in primary schooling is driven by delays is hopeful,

as it leaves room for rapid recovery. In terms of implications for future research, these findings suggest that empirical studies should give due attention to the time elapsed since the end of the conflict. It would for instance be possible to find a large negative impact of conflict on schooling in the early postwar years but no impact or even a positive impact further down the road. In any case, to better design post-war recovery programs, more case studies are needed to determine what underlies this finding and whether it is Rwanda-specific or more general.

Fourth, we provided new results on the effect of armed conflict on children from non-poor households (versus those from poor households) and on exposed boys (versus girls). Including all constitutive components of the interaction terms, and using a very large sample, we found no conclusive results regarding the heterogeneous impact across poor and non-poor households and an additional – albeit small – negative effect for exposed girls. Such accurate information on the heterogeneity of the impact is important for the targeting of post-war policies.

Fifth, we demonstrated that the drop in schooling cannot be attributed to the localized effects of the genocide, even when using data with complete geographic coverage and very fine genocide intensity measures. We ruled out that this result stems from pre-war regional trends, migration, selective killings, or targeted assistance to genocide survivors: correcting for these factors did not explain away the large drop in schooling, which is observed also in areas where very few Tutsi lived prior to the genocide.

At first sight, it may seem counterintuitive that one of the largest mass killing of the 21st century does not stand out in its effect on schooling, but we argued that there are reasons to assume that the impact of the genocide on the drop in schooling was nationwide. Not only did the genocide dry up the government budget, but it also triggered other disruptive events and several other forms of violence. Combined, these events affected every corner of the small

country, and - although very different in nature compared to the genocide - these conflicts may have been equally disruptive to schooling. This conjecture finds support in our finding that the commune-level share of households that were externally displaced is not correlated with any specific form of violence, while it does turn up as a determinant for part of the observed drop in schooling. Additional empirical support comes from the EICV data, which reveal no correlation between functioning school infrastructure and genocide intensity.

The conclusion armed conflict in Rwanda had a nationwide impact on schooling has relevant policy implications. Scholarships have been directed to genocide-affected areas and survivors of the genocide, being channeled through the various associations for survivors. Given the history of ethnic discrimination in Rwanda and its role in intensifying violence, such bias is not to be taken lightly. There is a danger that post-war reconstruction that is exclusively directed to genocide survivors might not only slow down the recovery of educational attainment, but may also reinforce group identities, potentially feeding ethnic grievances.

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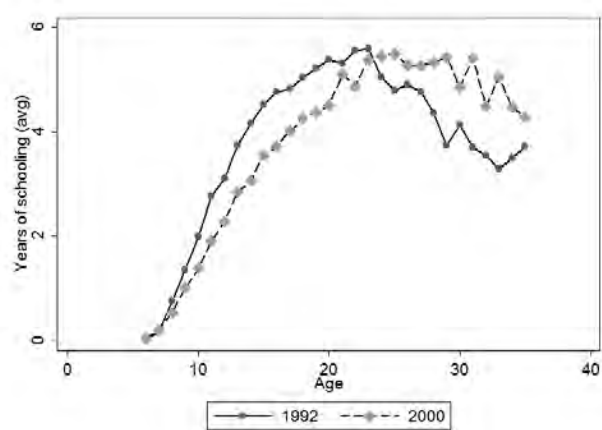
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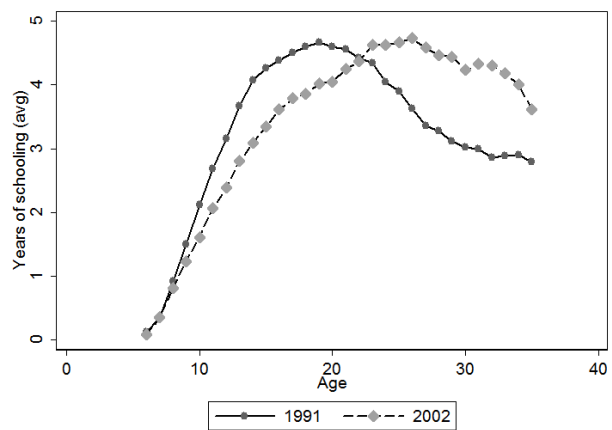
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Figure 1. Years of schooling, across all ages of the age group 6-35



a. DHS Data



b. Census Data

Figure 2. Years of schooling, across age and rural-urban

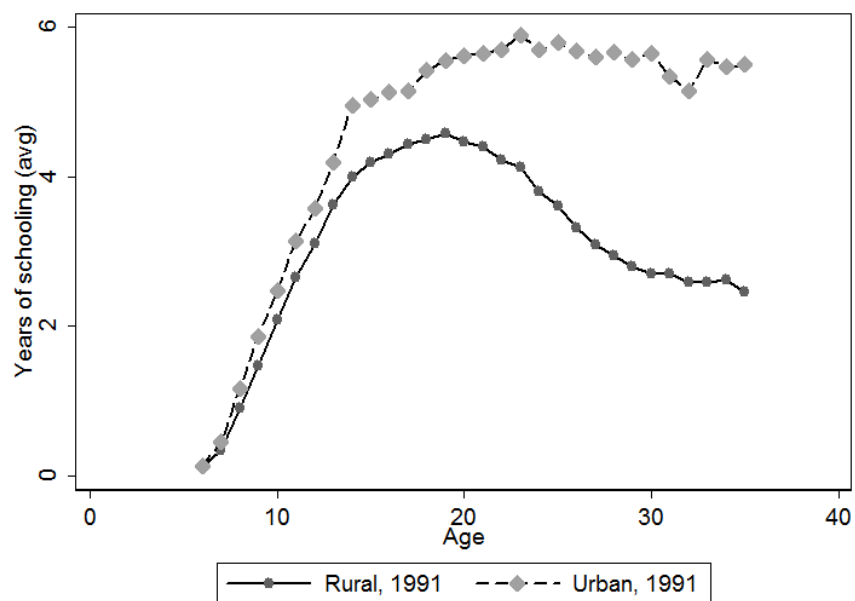


Figure 3. Share of students enrolled in primary and secondary school by age in 1991

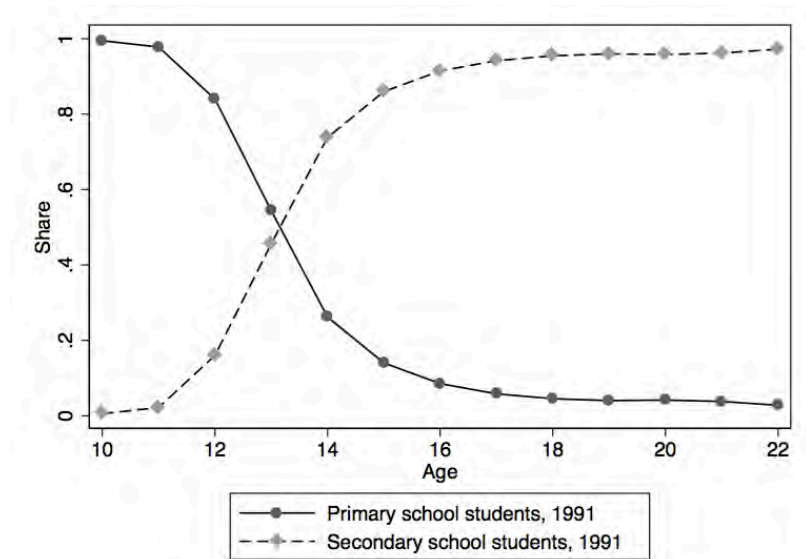
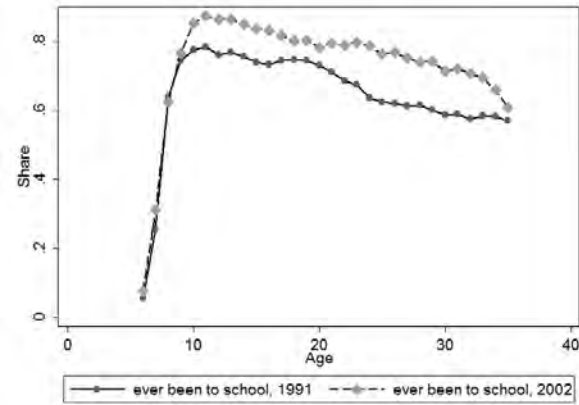
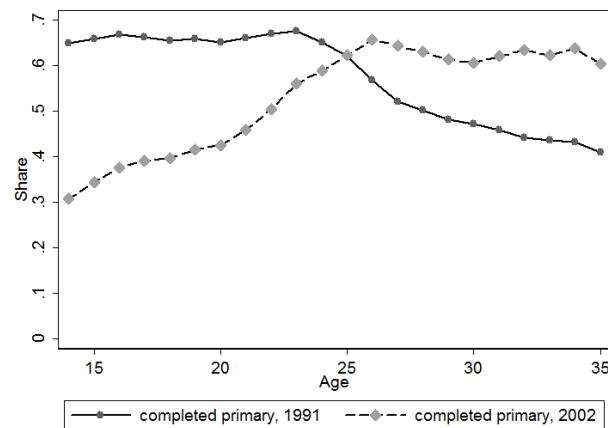


Figure 4. School initiation, drop-out or slow grade progression?

(a) Share of individuals that have ever been to school, by age



(b) Share of individuals that have completed primary school, among those that have ever attended it, but that are no longer enrolled in primary school, by age



(c) Average age by grade, for student population only

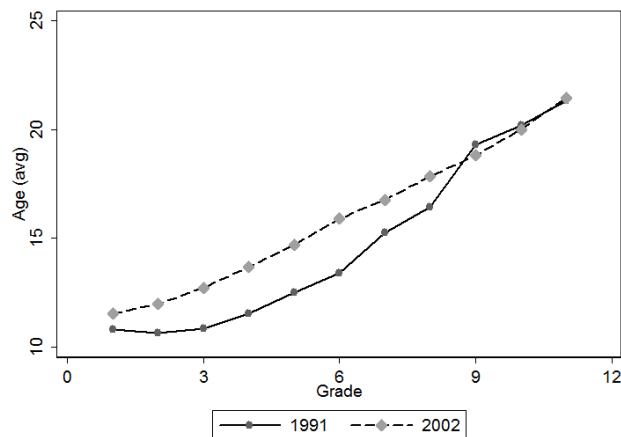
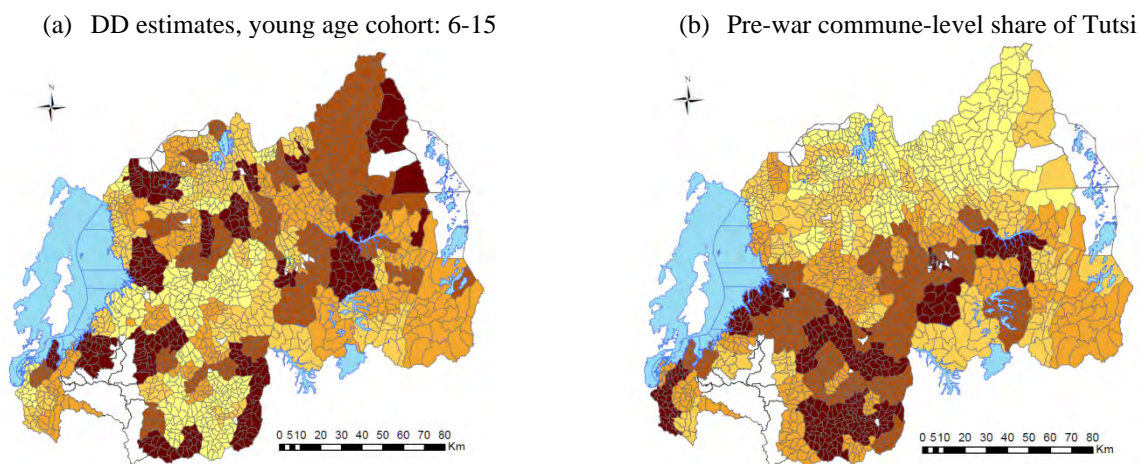


Figure 5. Comparison of the spatial pattern of DD estimates of the schooling deficit with the pre-genocide share of Tutsi in a commune



Top quintile (= largest schooling deficit or highest share of Tutsi) in darkest shade. The DD estimate is calculated for each commune separately, using $\log(\text{schooling})$ as a dependent variable to account for the different levels of schoolings between communes. The map is taken from a shape file of the Rwandan administrative sectors (which is one level below the communes, but for which no shape file exists). The areas in white are left out of the analysis. They include the national parks and forests.

Figure 6. Schooling across ethnicity in 1991

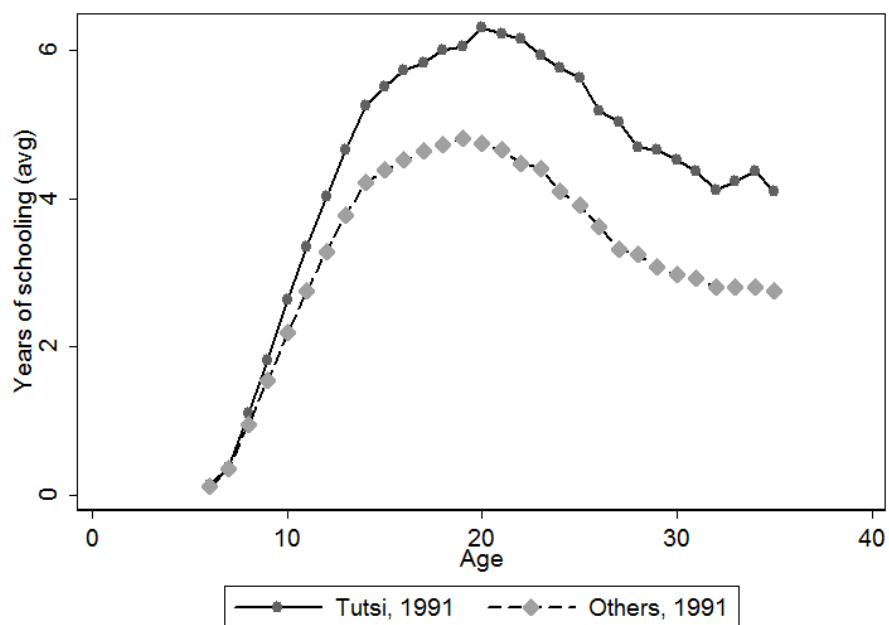


Figure 7. The relationship between the pre-war commune-level share of Tutsi and the share of students 6-35 enjoying a scholarship, as reported in a 1999/2000 nationwide survey

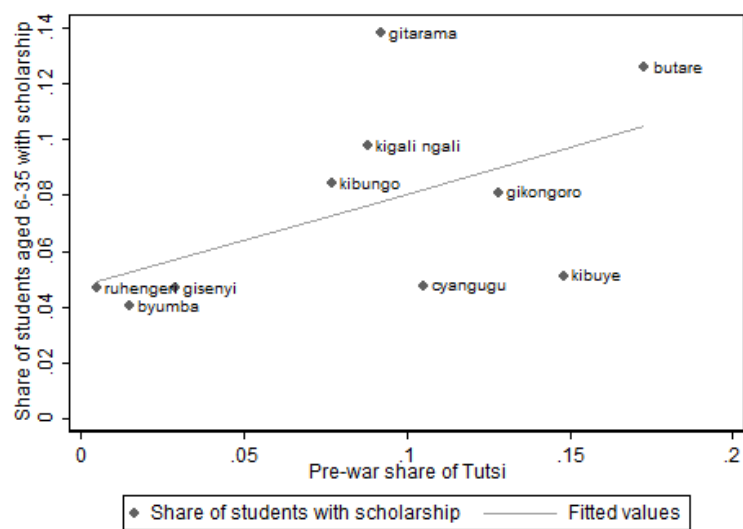


Table 1. Summary Statistics

Variable	Full Sample					Pre-conflict	Post-conflict
	Observations	Mean	Std Deviation	Min	Max	mean	mean
<i>PANEL A: DHS</i>							
Years of schooling	45400	3,296	3,293	0	21	3,420	3,212
Post-War Round	45400	0,597	0,491	0	1	0	1
Young Cohort	45400	0,489	0,500	0	1	0,477	0,497
Female	45400	0,523	0,499	0	1	0,514	0,529
Non-poor	45400	0,432	0,495	0	1	0,571	0,337
Age of HH Head	45400	42,975	13,938	7	97	44,049	42,249
Highest Education - Any HH Member	45400	5,121	4,006	0	21	5,369	4,954
Number of Children Under 5	45400	0,818	0,837	0	5	0,855	0,793
Rural	45400	0,781	0,414	0	1	0,830	0,747
Conflict Intensity (=Measure A)	45400	0,262	0,211	0,125	1	-	-
Conflict Intensity (=Measure B)	45400	0,305	0,460	0	1	-	-
Conflict Intensity (=Measure C)	45400	8,992	5,766	3	20	-	-
<i>PANEL B: Census</i>							
Years of schooling	906611	3,044	2,988	0	20	3,008	3,077
Post-War Round	906611	0,513	0,500	0	1	0	1
Young Cohort	906611	0,460	0,498	0	1	0,474	0,447
Female	906611	0,520	0,500	0	1	0,509	0,530
Non-poor	906611	0,554	0,497	0	1	0,482	0,622
Age of HH Head	906611	42,557	13,928	7	109	42,783	42,341
Highest Education - Any HH Member	906611	4,366	3,665	0	20	4,337	4,394
Number of Children Under 5	906611	0,913	0,895	0	9	1,008	0,823
Rural	906611	0,854	0,331	0	1	0,864	0,845
Been to School	884857	0,681	0,466	0	1	0,632	0,728
Been to Secondary	906611	0,145	0,352	0	1	0,177	0,114
Student	906611	0,246	0,431	0	1	0,169	0,320
Completed primary school	906611	0,239	0,426	0	1	0,254	0,224
Completed secondary school	906611	0,012	0,108	0	1	0,008	0,016
Conflict Intensity (=Measure A)	906611	23,923	19,563	12	96	-	-
Conflict Intensity (=Measure B)	906611	0,275	0,447	0	1	-	-
Conflict Intensity (=Measure C)	906611	8,780	6,000	3	20	-	-
Conflict Intensity (= Share Tutsi)	906611	0,114	0,119	0	0,6	-	-
Conflict Intensity (= Perpetrators)	906611	0,069	0,045	0,002	0,197	-	-
Conflict Intensity (= Mass Graves)	906611	0,489	0,802	0	4	-	-
Conflict Intensity (= (log) dist to Mass Grave)	906611	2,260	0,658	0,677	4,034	-	-
Conflict Intensity (=Excess Mortality Index)	906611	0,389	0,175	0	1	-	-
Conflict Intensity (=Extrajudicial Killings)	190941	0,415	0,649	0	2,287	-	-
Share of Displaced Households	906611	0,457	0,278	0,02	0,914	-	-
Migrant (8 year)	906611	0,164	0,370	0	1	0,118	0,208
Orphan	906611	0,228	0,420	0	1	0,137	0,314

Notes: For DHS data, pre-conflict refers to 1992 and post-conflict to 2000. For Census data, pre-conflict refers to 1991 and post-conflict to 2002. In both cases the sample is limited to individuals aged 6 to 35. See the paper for details concerning the specific variables.

Table 2. Narrow and Broad replication of basic result

Dependent Variable: Years of schooling	Replication					
	AdW	Replication		AdW	Replication	
		DHS data	Census data		DHS data	Census data
	(1)	(2)	(3)	(4)	(5)	(6)
Young Cohort * Post-war round	-0.555*** (0.116)	-0.504*** (0.131)	-0.573*** (0.032)	-0.421*** (0.097)	-0.494*** (0.102)	-0.649*** (0.026)
Post-War Round	0.123 (0.140)	0.092 (0.157)	0.278*** (0.039)	0.232*** (0.064)	0.231*** (0.065)	0.147*** (0.019)
Young Cohort	-2.249*** (0.085)	-2.541*** (0.104)	-1.776*** (0.069)			
<i>Controls X</i>						
Female				-0.113*** (0.026)	-0.074*** (0.026)	-0.134*** (0.024)
Non-poor				0.165*** (0.024)	0.294*** (0.031)	0.274*** (0.012)
Age of HH Head				0.005*** (0.001)	0.009*** (0.001)	0.008*** (0.000)
Highest Education - Any HH Member				0.446*** (0.006)	0.386*** (0.004)	0.363*** (0.003)
Number of Children Under 5				-0.113*** (0.013)	-0.116*** (0.016)	-0.051*** (0.010)
Rural				-0.270*** (0.103)	-0.408*** (0.047)	-0.156*** (0.035)
Child Age FE	No	No	No	Yes	Yes	Yes
Province FE	No	No	No	Yes	Yes	Yes
Observations	45,642	45,400	906,611	45,642	45,400	906,611
R-squared	n.a.	0.188	0.121	n.a.	0.521	0.425
Relative drop in schooling	n.a.	19.2%	16.2%	18.3%	19.5%	18.4%

Notes: Robust standard errors in parentheses, clustered at the enumeration level for the DHS data and at the commune level for the census data. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3. Poor versus non-poor

Dependent Variable: Years of schooling				
	AdW	DHS data	Census data	
	(1)	(2)	(3)	(4)
Young Cohort * Post-war round	0.160*	-0.729***	-0.511***	-0.468***
	(0.083)	(0.073)	(0.026)	(0.027)
Post-War Round	0.203***	0.338***	0.100***	0.077***
	(0.059)	(0.054)	(0.016)	(0.018)
Non-Poor * (Young Cohort * Post-war round)	-1.223***	-0.399***	-0.018	0.049
	(0.064)	(0.145)	(0.035)	(0.035)
Non-Poor * Young Cohort		-1.526***	-0.943***	-0.896***
		(0.110)	(0.069)	(0.046)
Non-Poor * Post-war round		0.149	-0.030	-0.087***
		(0.094)	(0.025)	(0.024)
Urban * (Young Cohort * Post-war round)				-0.118
				(0.158)
Urban * Young Cohort				-0.724***
				(0.219)
Urban * Post-war round				0.341***
				(0.064)
Controls X	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Observations	45,642	45,400	906,611	906,611
R-squared	n.a.	0.537	0.431	0.433

Notes: Robust standard errors in parentheses, clustered at the enumeration level for the DHS data and at the commune level for the census data. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4. Girls versus boys

Dependent Variable: Years of schooling				
	AdW	DHS data	Census data	
	(1)	(2)	(3)	(4)
Young Cohort * Post-war round	- 0.535*** (0.101)	-0.478*** (0.115)	-0.578*** (0.033)	-0.466*** (0.025)
Post-War Round	0.235*** (0.064)	0.230*** (0.079)	0.044 (0.028)	-0.025 (0.021)
Female * (Young Cohort * Post-war round)	0.219*** (0.044)	-0.039 (0.098)	-0.141*** (0.027)	-0.149*** (0.026)
Female * Young Cohort		0.252*** (0.081)	0.331*** (0.033)	0.329*** (0.033)
Female * Post-war round		0.009 (0.086)	0.205*** (0.026)	0.215*** (0.023)
Urban * (Young Cohort * Post-war round)				-0.084 (0.191)
Urban * Young Cohort				-0.962*** (0.272)
Urban * Post-war round				0.345*** (0.080)
Controls X	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Observations	45,642	45,400	906,611	906,611
R-squared	n.a.	0.521	0.426	0.429

Notes: Robust standard errors in parentheses, clustered at the enumeration level for the DHS data and at the commune level for the census data. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5. Alternative age cohorts and the impact on secondary schooling

Dependent Variable: Years of schooling						
	<i>Young Cohort:</i>	6-15 years	8-17 years	6-22 years	6-13 years	14-22 years
	<i>Old Cohort:</i>	16-35 years	18-37 years	23-35 years	23-35 years	23-35 years
		(1)	(2)	(3)	(4)	(5)
Young Cohort * Post-war round		-0.649*** (0.026)	-0.919*** (0.029)	-1.176*** (0.031)	-1.104*** (0.030)	-1.073*** (0.033)
Post-War Round		0.147*** (0.019)	0.299*** (0.017)	0.681*** (0.023)	0.690*** (0.021)	0.574*** (0.017)
Controls X		Yes	Yes	Yes	Yes	Yes
Child Age FE		Yes	Yes	Yes	Yes	Yes
Province FE		Yes	Yes	Yes	Yes	Yes
Observations		906,611	839,476	906,611	600,753	566,441
R-squared		0.425	0.392	0.430	0.465	0.421
Relative drop in schooling		18,4%	21,1%	24,8%	27,5%	17,4%

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6. School initiation, slow grade progression and drop-outs

	(1)	(2)	(3)
PRIMARY SCHOOLING , Dependent Variable:	Been to school	Completed primary	Age Students
<i>Young Cohort:</i>	6-15	14-22	10-18
<i>Old Cohort:</i>	16-35	23-35	
Young Cohort * Post-war round	-0.027*** (0.004)	-0.281*** (0.006)	
Post-War Round	0.090*** (0.003)	0.069*** (0.004)	1.694*** (0.028)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	No
Grade FE	No	No	Yes
Province FE	Yes	Yes	Yes
Observations	906,611	377,051	132,852
R-squared	0.276	0.250	0.312
SECONDARY SCHOOLING , Dependent Variable:	Been to secondary	Completed secondary	Age Students
<i>Young Cohort:</i>	14-22	18-26	18-26
<i>Old Cohort:</i>	23-35	27-35	
Young Cohort * Post-war round	-0.285*** (0.007)	0.048*** (0.006)	
Post-War Round	-0.010 (0.011)	0.067*** (0.004)	0.225*** (0.033)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	No
Grade FE	No	No	Yes
Province FE	Yes	Yes	Yes
Observations	209,984	69,766	15,866
R-squared	0.296	0.555	0.127

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%. PANEL A: the sample considered in the second column is restricted to individuals that have completed at least one year of primary schooling, but that are not enrolled in primary school any more; the sample considered in the third column is restricted to students attending class 1 to 6. PANEL B: the sample considered in the first column is restricted to individuals that have completed primary school; the sample considered in the second column is restricted to individuals that have completed at least one year of secondary schooling, but that are not enrolled in secondary school any more; the sample considered in the third column is restricted to students attending class 7 to 12.

Table 7. Triple differenced regression with province-level conflict intensity measures

Dependent Variable: Years of schooling Conflict Intensity Measure:	Panel A: AdW			Panel B: DHS data			Panel C: Census data		
	A	B	C	A	B	C	A	B	C
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Conflict Intensity * (Young * Post-war)	-0.024* (0.014)	-0.329* (0.196)	-0.023* (0.013)	0.744** (0.296)	-0.892*** (0.220)	0.005 (0.016)	0.002* (0.001)	0.079 (0.060)	0.004 (0.003)
Conflict Intensity * Young Cohort	n.a.	n.a.	n.a.	-0.062 (0.238)	0.176 (0.164)	0.022* (0.012)	-0.002 (0.002)	0.012 (0.083)	-0.003 (0.005)
Conflict Intensity * Post-war round	n.a.	n.a.	n.a.	-0.611*** (0.227)	0.317** (0.142)	-0.020* (0.011)	-0.001 (0.001)	-0.021 (0.035)	-0.007*** (0.002)
Young Cohort * Post-war round	n.a.	n.a.	n.a.	-0.688*** (0.140)	-0.230** (0.117)	-0.529*** (0.187)	-0.707*** (0.036)	-0.668*** (0.031)	-0.689*** (0.032)
Post-War Round	n.a.	n.a.	n.a.	0.391*** (0.094)	0.123* (0.074)	0.412*** (0.124)	0.174*** (0.026)	0.151*** (0.022)	0.207*** (0.025)
Controls X	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45,642	45,642	45,642	45,400	45,400	45,400	906,611	906,611	906,611
R-squared	n.a.	n.a.	n.a.	0.521	0.524	0.521	0.425	0.425	0.425

Notes: Robust standard errors in parentheses, clustered at the enumeration level for the DHS data and at the commune level for the census data. All conflict Intensity Measures are defined at the Province level. *Measure A* is represented by the proportion of days during which killings occurred in a province in the months April-June 1994; *Measure B* is instead an indicator variable taking one for the three provinces with the highest number of killings in 1994; finally *Measure C* records the number of mass graves and memorials per province. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8. Triple differenced regression with commune-level conflict intensity measures

Dependent Variable: Years of schooling				
Conflict Intensity Measure:	Share Tutsi (1)	Share Perpetrators (2)	Mass Graves (3)	Distance to Mass Grave (4)
Conflict Intensity * (Young * Post-war)	0.060 (0.191)	0.719 (0.484)	-0.024 (0.038)	-0.031 (0.037)
Conflict Intensity * Young Cohort	-1.268*** (0.362)	-1.029 (1.058)	-0.160*** (0.059)	0.375*** (0.085)
Conflict Intensity * Post-war round	0.035 (0.142)	-0.752*** (0.282)	0.007 (0.026)	0.016 (0.029)
Young Cohort * Post-war round	-0.672*** (0.026)	-0.703*** (0.038)	-0.642*** (0.025)	-0.596*** (0.092)
Post-War Round	0.153*** (0.020)	0.200*** (0.028)	0.145*** (0.018)	0.121* (0.072)
Conflict Intensity (= Share Tutsi)	0.893*** (0.225)			
Conflict Intensity (= Perpetrators)		1.415** (0.664)		
Conflict Intensity (= Mass Graves)			0.083** (0.032)	
Conflict Intensity (= Distance to Mass Grave - log)				-0.257*** (0.048)
Controls X	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Observations	906,611	906,611	906,611	906,611
R-squared	0.426	0.425	0.426	0.427
DD High Conflict intensity	-0.670***	-0.595***	-0.717***	-0.649***
DD Low Conflict Intensity	-0.659***	-0.694***	-0.624***	-0.661***

Notes: Robust standard errors in parentheses, clustered at the commune level. All conflict Intensity Measures are defined at the Commune level. *Share Tutsi* is the proportion of Tutsi living in the commune in the pre-war population; *Share Perpetrators* is the share of genocide suspects identified in the Commune; *Mass Graves* is the number of mass graves in the Commune; *Distance to Mass Grave* is and the distance to the nearest mass grave calculated from the commune centroid. The last two rows of the table report the DD coefficients obtained through regressions restricted to communes associated to a conflict intensity variable above or below the median. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9. Confounding factors

Dependent Variable: Years of schooling	Panel A: Include province factors		Panel B: Assign migrants to previous commune		Panel C: Drop 75% Tutsi in 1991		Panel D: Drop best educated individuals in young cohort proportional to scholarships	
	DD	DDD	DD	DDD	DD	DDD	DD	DDD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young Cohort * Post-war round	-0.566*** (0.081)	-0.622*** (0.100)	-0.614*** (0.025)	-0.639*** (0.025)	-0.699*** (0.029)	-0.699*** (0.028)	-0.791*** (0.027)	-0.785*** (0.027)
Post-War Round	0.120*** (0.045)	0.137** (0.054)	0.114*** (0.021)	0.122*** (0.021)	0.209*** (0.021)	0.176*** (0.021)	0.147*** (0.019)	0.153*** (0.020)
Conflict Intensity * (Young * Post-war)		0.189 (0.232)		0.136 (0.187)		-0.042 (0.210)		-0.230 (0.219)
Conflict Intensity * Young Cohort		-1.064*** (0.249)		-1.260*** (0.356)		-1.170*** (0.365)		-1.258*** (0.362)
Conflict Intensity * Post-war round		-0.038 (0.150)		0.033 (0.165)		0.331** (0.163)		0.037 (0.141)
Conflict Intensity (= Share Tutsi)		0.800*** (0.205)		0.883*** (0.214)		0.607*** (0.228)		0.924*** (0.224)
Controls X	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full set of province interactions	Yes	Yes	No	No	No	No	No	No
Observations	906,611	906,611	864,315	856,689	879,822	879,822	891,047	891,047
R-squared	0.429	0.429	0.421	0.421	0.423	0.423	0.430	0.431

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%. The conflict Intensity variable considered in the regression is the Share of Tutsi living in the Commune in 1991. Regressions in Panel A include a complete set of province trends and province interaction terms (i.province*young*postwar, i.province*young, i.province*postwar). Regressions in Panel B consider individuals that migrated between 1994 and 2002 as belonging to their commune of origin. Regressions in Panel C exclude from the 1991 population 75% of Tutsi, randomly selected. Regressions in Panel D exclude from each province a share of individuals in the young cohort of the 2002 population census equal to the share of students reported to have a scholarship according to the EICV1 dataset.

Table 10. Alternative forms of violence & displacement

Dependent Variable: Years of schooling	Panel A: Non-genocide excess mortality index		Panel B: Extrajudicial killings in the NorthWest		Panel C: Displacement	
	DD	DDD	DD	DDD	DD	DDD
	(1)	(2)	(3)	(4)	(5)	(6)
Young Cohort * Post-war round	-0.649*** (0.026)	-0.619*** (0.055)	-0.623*** (0.047)	-0.596*** (0.052)	-0.649*** (0.026)	-0.581*** (0.041)
Post-War Round	0.147*** (0.019)	0.163*** (0.031)	0.124*** (0.040)	0.127** (0.048)	0.147*** (0.019)	0.136*** (0.025)
Conflict Intensity * (Young * Post-war)		-0.078 (0.132)		-0.066 (0.051)		-0.145** (0.071)
Conflict Intensity * Young Cohort		-0.320 (0.258)		0.051 (0.104)		-0.083 (0.163)
Conflict Intensity * Post-war round		-0.045 (0.082)		-0.008 (0.034)		0.022 (0.054)
Conflict Intensity (=Excess Mortality Index)		0.127 (0.169)				
Conflict Intensity (=Extrajudicial Killings)				-0.015 (0.053)		
Share of Displaced Households						-0.046 (0.116)
Controls X	Yes	Yes	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	906,611	906,611	190,941	190,941	906,611	906,611
R-squared	0.425	0.425	0.420	0.420	0.425	0.425
DD High Conflict intensity / Share displaced HHs		-0.694***		-0.651***		-0.715***
DD High Conflict intensity / Share displaced HHs		-0.594***		-0.599***		-0.550***

Notes: Robust standard errors in parentheses, clustered at the commune level. Panels A and B consider alternative conflict intensity proxies. The conflict Intensity variable considered in Panel A is the excess mortality caused by episodes of violence different from the genocide, as constructed by Verpoorten (2012). The conflict Intensity variable considered in Panel B is the number of extrajudicial killings of civilians during the (counter)insurgency, collected from four Amnesty International Reports. In Panel B the sample is restricted to the provinces of Gisenyi and Ruhengeri, where (counter)insurgency took place. Panel C considers instead the share of displaced HHs, defined as the 2002 share of households that have at least a child that was born in DRC, Tanzania and Burundi between 1994 and 1998 and all the adult members were born in Rwanda, computed over the HHs that have at least a child born over that period. The variable is computed from the census and is defined at the commune level. The last two rows of the table report the DD coefficients obtained through regressions restricted to communes associated to a conflict intensity variable or a share of displaced HHs above or below the median. * significant at 10%, ** significant at 5%, *** significant at 1%.

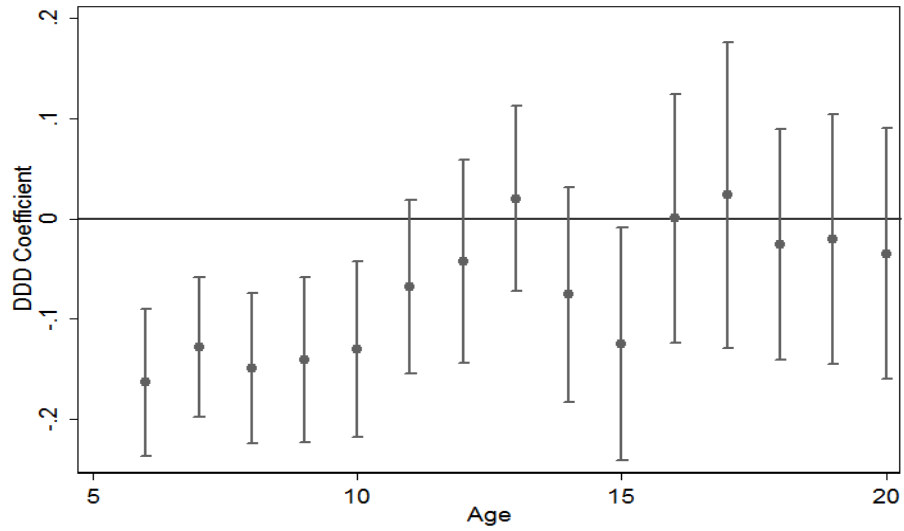
Table 11. Alternative forms of violence

Province	DD estimate	Justino and Verwimp		
		Genocide	Civil War	Insurgency
<i>Byumba</i>	-0,698	0	1	0
<i>Gikongoro</i>	-0,694	2	0	0
<i>Cyangugu</i>	-0,682	2	0	0
<i>Kigali (rural)</i>	-0,676	1	1	0
<i>Gisenyi</i>	-0,667	0	0	1
<i>Kigali City</i>	-0,656	na	na	na
<i>Ruhengeri</i>	-0,589	0	1	1
<i>Kibungo</i>	-0,568	1	1	0
<i>Gitarama</i>	-0,553	1	0	0
<i>Butare</i>	-0,545	2	0	0
<i>Kibuye</i>	-0,417	2	0	0

Note: The DD estimates are obtained by running the baseline regression by province. Justino and Verwimp (2008) provide a qualitative assessment of the intensity of genocide, civil war and (counter-)insurgency, obtained by combining different sources.

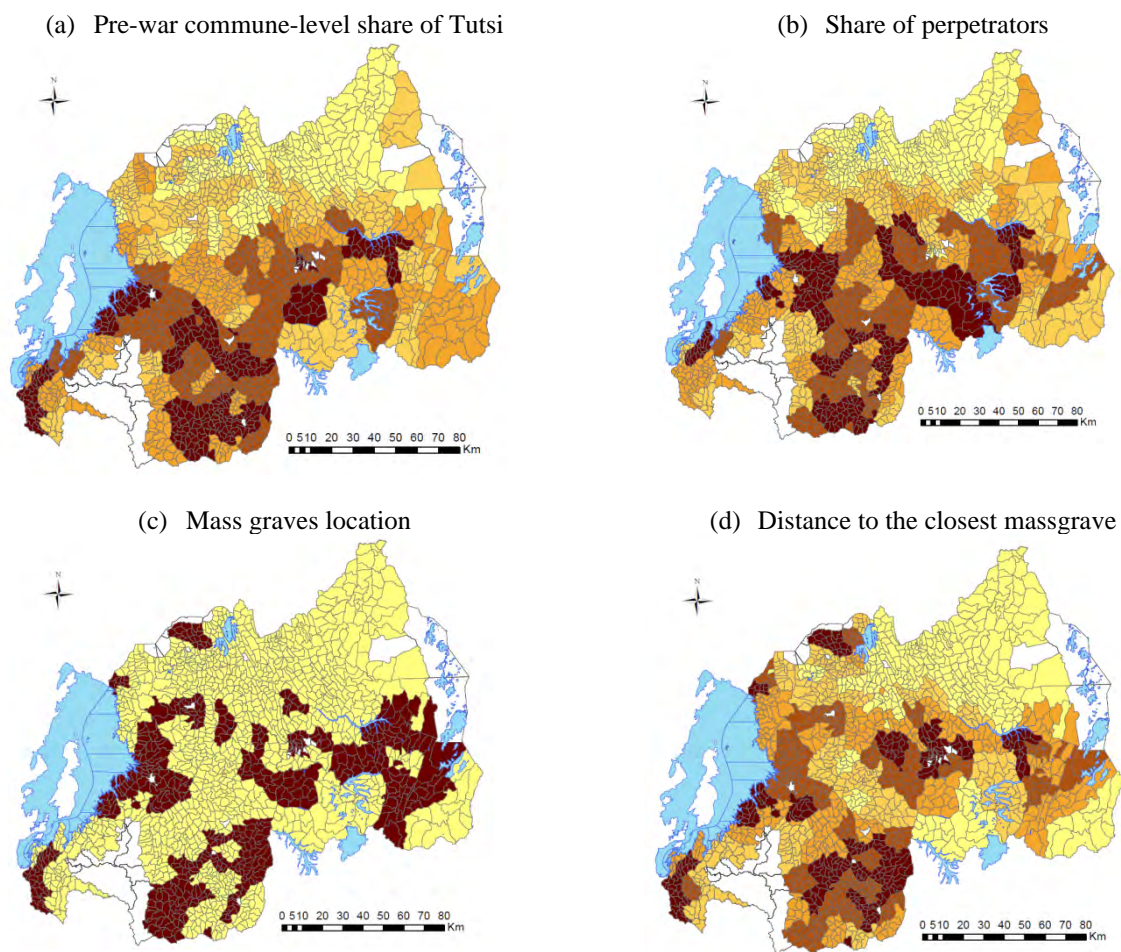
APPENDIX

Figure A1. DDD coefficient by age



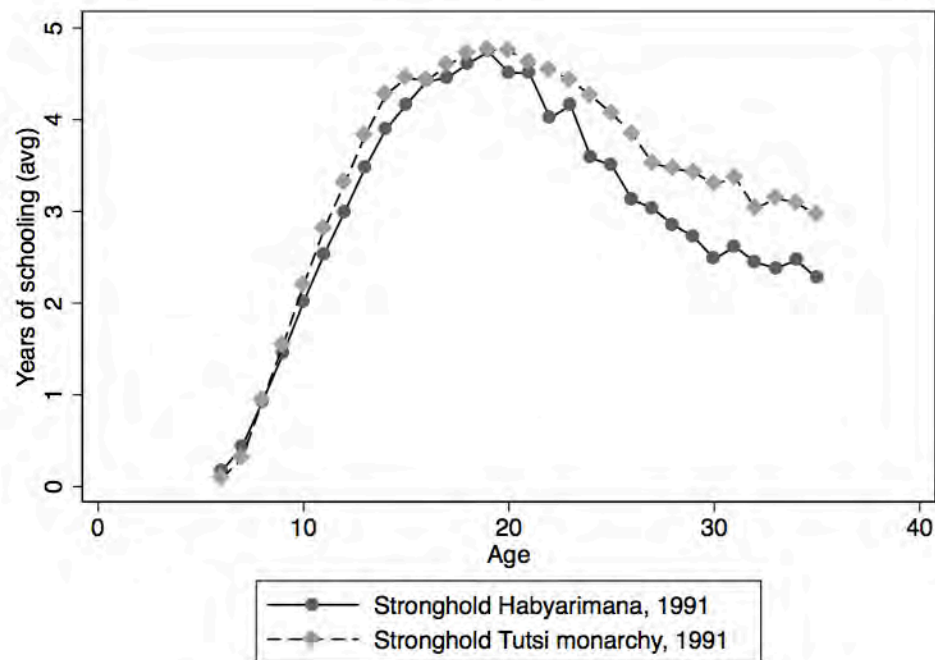
Notes: The figure shows the point estimates and the 95% CI for the DDD coefficients of the interaction terms obtained from a regression similar to the one reported in column (3) of Table 4, in which the *Young* cohort has been replaced by the individual years, from 6 to 20 i.e. all the triple interactions *female*postwarround*age6* etc are included, as well as all partial interactions.

Figure A2. Comparison of the spatial pattern of the different genocide intensity proxies



Top quintile in darkest shade. For figure (c) communes in which at least one massgrave was located are coloured in dark shade, while those in which no mass grave was found are in light shade. In figure (d) a darker shade indicates that the (centroid of the) commune was closer to a massgrave. The map is taken from a shape file of the Rwandan administrative sectors (which is one level below the communes, but for which no shape file exists). The areas in white are left out of the analysis. They include the national parks and forests.

Figure A3. Regional trends in schooling in 1991



Notes: The stronghold of Habyarimana includes the northwestern provinces Gisenyi and Gitarama. The stronghold of the Tutsi monarchy covers Butare and Gitarama.

Figure A4. Years of schooling, across age and migrants vs. non-migrants

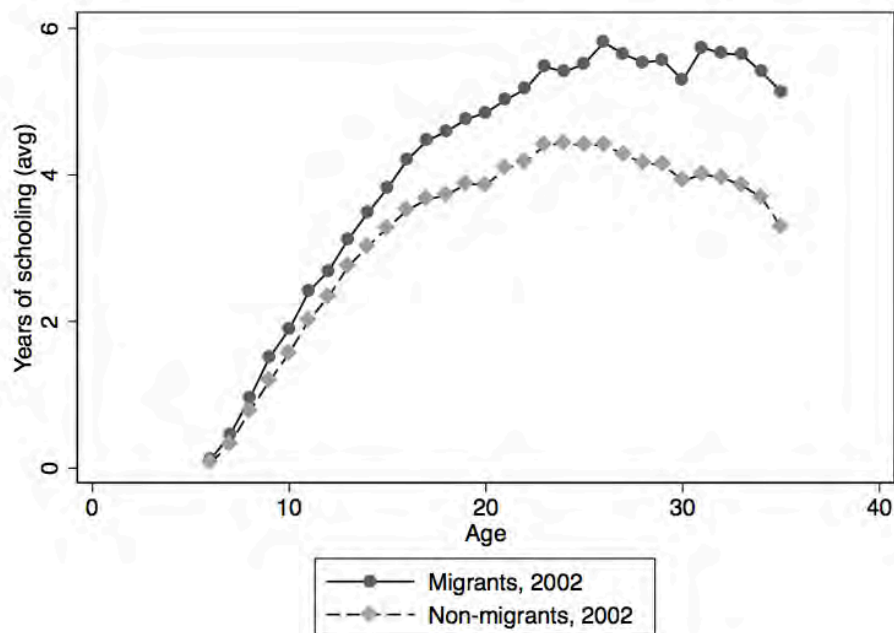


Figure A5. Years of schooling, across age, high or low share of Tutsi in 1991 and paternal orphans vs. others

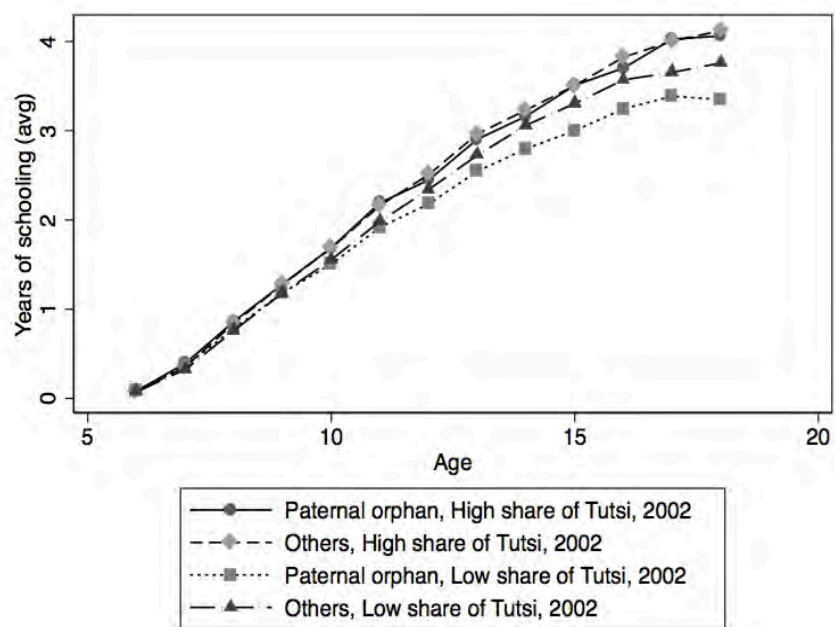


Table A1. School initiation, slow grade progression and drop-outs – girls versus boys

	(1)	(2)	(3)
PRIMARY SCHOOLING , Dependent Variable:	Been to school	Completed primary	Age Students
<i>Young Cohort:</i>	6-15	14-22	10-18
<i>Old Cohort:</i>	16-35	23-35	
Young Cohort * Post-war round	-0.008** (0.004)	-0.269*** (0.006)	
Post-War Round	0.063*** (0.002)	0.067*** (0.004)	1.723*** (0.029)
Female * (Young Cohort * Post-war round)	-0.037*** (0.004)	-0.122*** (0.008)	
Female * Young Cohort	0.060*** (0.004)	0.052*** (0.006)	
Female * Post-war round	0.052*** (0.003)	0.003 (0.004)	-0.057*** (0.016)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	No
Grade FE	No	No	Yes
Province FE	Yes	Yes	Yes
Observations	906,611	377,051	132,852
R-squared	0.277	0.251	0.312
SECONDARY SCHOOLING , Dependent Variable:	Been to secondary	Completed secondary	Age Students
<i>Young Cohort:</i>	14-22	18-26	18-26
<i>Old Cohort:</i>	23-35	27-35	
Young Cohort * Post-war round	-0.315*** (0.009)	0.068*** (0.007)	
Post-War Round	0.022*** (0.006)	0.052*** (0.006)	0.176*** (0.043)
Female * (Young Cohort * Post-war round)	0.073*** (0.010)	-0.043*** (0.007)	
Female * Young Cohort	-0.090*** (0.007)	0.044*** (0.007)	
Female * Post-war round	-0.076*** (0.007)	0.034*** (0.009)	0.113* (0.061)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	No
Grade FE	No	No	Yes
Province FE	Yes	Yes	Yes
Observations	205,678	69,766	15,866
R-squared	0.299	0.556	0.127

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%. PANEL A: the sample considered in the second column is restricted to individuals that have completed at least one year of primary schooling, but that are not enrolled in primary school any more; the sample considered in the third column is restricted to students attending class 1 to 6. PANEL B: the sample considered in the first column is restricted to individuals that have completed primary school; the sample considered in the second column is restricted to individuals that have completed at least one year of secondary schooling, but that are not enrolled in secondary school any more; the sample considered in the third column is restricted to students attending class 7 to 12.